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*EXPERIMENTS ON THE DEVIATIONS OF FALLING
BODIES.*

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EXPERIMENTS ON THE DEVIATIONS OF FALLING BODIES.

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THIS paper is a supplement to one recently published in the *Physical Review* * under the title, "Do Falling Bodies move South?" It will give some account of the preliminary experiments and variations of method which the author made in the course of his study of the behavior of falling bodies, and will discuss to some extent sources of possible error in the result arrived at; a result which is in favor, so far as it carries weight, of an affirmative answer to the question stated above. It is unlikely that such details as are here to be given will interest any large number of readers, but they may be of considerable value to any investigator who may hereafter occupy himself with this still unsettled question.

Cajori remarks that all experimenters in this research have used metal balls, and therefore I, desiring to vary the conditions, undertook at first to use spheres of ivory, about 2.54 cm. in diameter. They were made by a manufacturer of billiard balls, and were, presumably, good specimens of workmanship in ivory, but under the calipers they showed themselves not to be true spheres, differences of diameter along different axes being very perceptible, as large, perhaps, as 0.01 cm. A number of experiments were made in dropping these balls; but they were so erratic in their fall that I felt obliged to give up the hope of getting any valuable result from them, and I therefore resorted to balls of bell-metal, which, to my surprise, I found already in the hardware market, almost perfect in form.

It now seems probable that the unsatisfactory performance of the ivory balls was in part due to a cause which at first gave me much trouble in

* Part I., *Historical*, in September, 1903; Part II., *Methods and Results* of the Author's work, October, 1903.

the use of the metal balls, an occasional slight adhesion of the ball to one side of the hole from the bottom of which it was released (see top of

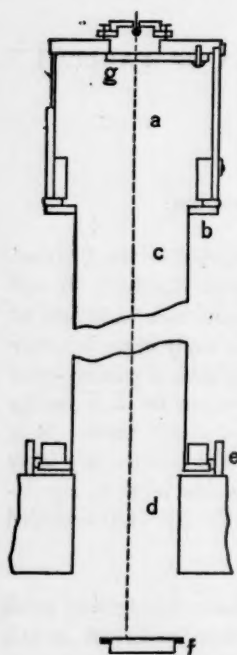


FIGURE 1.

Fig. 1), due to the accidental presence there of a bit of wax intended for use on the suspending thread. This source of error was discovered April 17, 1902, and wax was thereafter avoided. Moreover, as each ball, in spite of considerable care given to making it clean before dropping, might possibly leave some small amount of grease adhering to its circular line of contact with the release plate, this part of the plate was, in all later work, wiped after the dropping of each ball. The usual instrument employed for this purpose was a button of cork fastened at the lower end of a wire.

Previously I had attributed the occasional very bad behavior of a ball to a jerk given by a stray fibre of the silk suspending thread catching on some little roughness of the neighboring metal; and it is probable that some of the irregularities observed were due to this cause. More than once I heard a sharp *clink* from the pull of a fibre when a suspended ball was slowly revolving. It was to prevent such complications that wax had been used on the thread. Fine copper wire was for a time used in place of silk thread for the suspension. It soon appeared, however, that the time required for the melting off of the wire, though short, was long enough to allow the ball, before full release, to swing off a trifle from the side on which the flame was applied to the wire. On April 23 and thereafter, fine cotton thread was used for the suspension, in the hope that its fibres would prove less troublesome than those of the silk; but, as an additional precaution, that part of the thread from which trouble was feared was singed, just before the ball was put into position, by passing it very quickly through a Bunsen flame.

After these changes of practice had been made, cases of exceedingly bad behavior on the part of the balls, such as occurred often before, were very infrequent, and I was henceforth able to make no great improvement in their performance. It has already been stated that

considerable irregularity marked their behavior to the very end of the investigation. This irregularity was probably due to a variety of causes, among which the most obvious were, initial sidewise motion, due to vibrations of the tower, and variations of hardness in the arresting mixture. The method of release used, or, rather, the method of fixing and controlling the ball just before release, is evidently one which makes the ball share any accidental slight movements of the top of the tower, such as are maintained almost constantly during the day, and much of the time at night, by disturbances coming through the ground from machinery or street-cars or athletic exercises in the vicinity of the laboratory. Difficulty from this cause was reduced by working at night, and by releasing the ball at an instant when a mercury surface, in a glass vessel resting on the top of the box *a* (Fig. 1), gave an apparently undisturbed image of crossed threads attached to the side of the vessel.

At one time, about the middle of April, the experiment of releasing the ball from a condition of free suspension just beneath the upper plate was tried. This method required sharp scrutiny of the ball or of the suspending thread to determine whether pendulum movements of serious magnitude existed before release, and was therefore very trying to the nerves, besides being comparatively slow. Moreover, the pendulum movements never did quite die out, and the performance of balls dropped from a condition of free suspension seemed no better, on the whole, than that of balls dropped from the condition of suspension controlled by contact. Before April 26 I abandoned the method of free suspension.

April 18, I tried another experiment, using one of the bell-metal balls as a counterpoise for the similar ball to be dropped, and making the contact of the latter with the plate as light as it could well be made. This method of procedure also was tedious and not particularly successful, so that it was soon given up. Once or twice afterward, on May 5, for example, one of the balls was used as a counterpoise, no unusual attention being paid to the character of the contact, which of course must have been rather light; but I presently came to the conclusion that better results were obtained by means of the usual counterpoise, which weighed 93 gm., about 20 gm. more than the weight of a ball. This held the ball firmly in place till the thread was burned, and though the pressure was doubtless in many cases considerably greater on one side of the circle of contact than on the other, any lateral thrust from this source after the burning of the thread must have lasted an exceedingly short time, too short for any harm. The method of release to which I finally settled down, and which has now been sufficiently described, has

the merit of being very expeditious, the twenty-four balls constituting an evening's tale being, as a rule, dropped in less than one hour and a half.

At the receiving end also of the apparatus there was considerable experimenting. At first the pans were not fastened in the supporting seat in such a way as to preclude the possibility of rotation under the shock of the balls, and it is quite possible that slight rotations did occur, sometimes in one direction and sometimes in the other, their net effect being presumably zero. A locking device for preventing accidental rotation was first used April 7; but until May 21, the screw then adopted as a part of the lock in Fig. 2, was not employed, a wedge being inserted in the lock beside the stud when it appeared to be needed.

Previous to April 16, but not later, the receiving pan (see Figs. 4 and 5), after catching ball No. 1, was turned counter-clockwise to take position for ball No. 2, in the same direction for ball No. 3, and so on. This practice dropped the 1st and also the 6th ball into a field symmetrical as to its north and south aspects; but it dropped the 2d, 3d, 4th, and 5th balls into a field having a ball already lodged in the southerly quarter, with no corresponding ball in the northerly quarter. The average clear distance between neighboring balls in the pan was probably rather more than 3.5 cm., and it seemed unlikely that, as a rule, the lodgment of a ball in the tallow would be appreciably affected by the lodgment of an earlier ball 3.5 cm. away; but in some cases the tallow was inclined to crumble for a considerable distance about each ball, and in such cases there was sometimes a crumbling all the way through from a ball to its nearest neighbor. Evidently the practice of turning the pan always in such a direction as to make the majority of the balls fall to the north of their nearest predecessors might make the crumbling more prevalent on the south than on the north of the balls at the critical instant of lodgment; and therefore the displacement of the balls due to this crumbling might show a balance toward the south. To avoid this possibility the practice was adopted of rotating the pan first in one direction and then in the other, so that, after April 16, the order of arrangement of the six balls in any pan corresponded to the order of arrangement of the six studs projecting from the pan, as numbered in Fig. 5.

Too little attention was paid, at first, to the telescopes with which observations on the plumb-line and on the point above the balls (see Fig. 4) were made. As the depth to which any ball would sink in the tallow was somewhat uncertain, it was not possible to adjust the telescopes in advance so that the tip of the plug above the ball would fall just at the

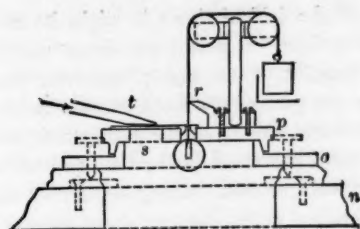


FIGURE 2.

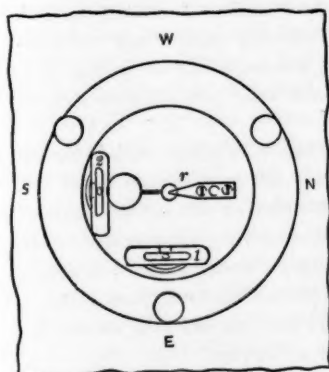


FIGURE 3.

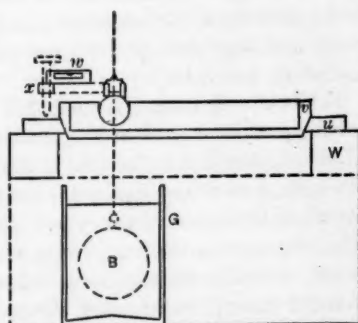


FIGURE 4.

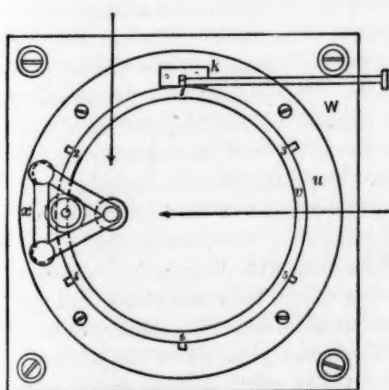


FIGURE 5.

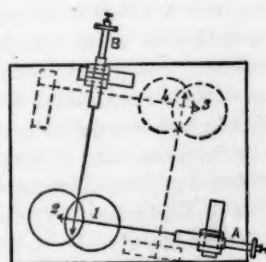


FIGURE 6.

level of the centre of the field of view; it might be two or three millimeters too high or too low. It would not do to readjust the telescope for each ball; and therefore it was highly important that the micrometer cross-hair, which in one part of the work was set on the plumb-line and in another part was set on the tip of the plug, should be strictly vertical. This condition was readily attained during the observations on the plumb-line, with which the work of each evening began; but the jar of the balls dropping into the pans had a tendency to loosen for a moment the fastening of the telescope, and during this instant the unbalanced weight of the micrometer screw, etc., sometimes caused a slight rotation of the instrument, with corresponding inclination of the cross-hair from the vertical. The full measure of the danger of error from this quarter was not realized before April 23; but after that date, and especially after May 23d, much care was given to the telescopes, and their performance was, on the whole, satisfactory.

It has already been intimated that the tallow mixture used in the receiving pans gave more or less trouble. At the temperature about 15° C., which prevailed in the base of the tower during the earlier weeks of the work, pure tallow was rather too hard, crumbling too much or cracking under the impact of the balls. To remedy this defect lard was added. After some weeks the temperature in this part of the tower rose a degree or two, and then the mixture of tallow and lard was too soft, so that the balls fell through it and struck the bottom of the pan.

Then beeswax, added to the tallow and lard, was used to give the right degree of hardness. The necessity of such adjustments was a little troublesome, but there were more serious difficulties. No mixture which was used appeared to be homogeneous when cold. All were more or less mottled, whiter in some places than in others, and the whiter spots appeared to be softer than the others. This condition of the arresting medium probably accounts in some measure for the irregularities of position of the arrested balls, the mixture giving way more readily on one side of a ball than on the other side. Such irregularities, however, must in the long run come to naught, if the variations of hardness are purely accidental in their distribution. They prolong the research, but do not affect the final result. Variation of hardness with distance from the edge of the pan has already (in a preceding paper) been mentioned, and the means taken for eliminating, in some measure, error from this source have been described. Such error would apparently not affect the southerly deviation of the balls; but it probably does affect to some extent even the final result along the east and west line; for the behavior of the tal-

low could not be kept uniform during all the changes of position of the pans indicated by Fig. 6. Accordingly, I regard the mean value found for the easterly deviation as entitled to considerably less confidence than the mean value found for the southerly deviation.

At times the tallow mixture adhered well to the bottom and side of the pan. At other times it would loose hold on the pan, during cooling, to such an extent as to be easily removable in a single cake. To prevent error from this condition it was the practice, whenever there was a gap between the tallow and the side of the pan, to force slips of paper on thin cardboard into the gap in such a way as to pack the tallow in place. Moreover the studs, which (see Fig. 5), on and after April 7, projected from the outside of the edge of the pan, were really the heads of screws which extended a considerable distance into the tallow, and these screws helped much to hold the tallow from rotation or sliding.

On July 10 soft wood (white wood) about 2 cm. thick was used in two of the pans instead of tallow. The mean of the range of balls in these two pans on this night was decidedly less, in both latitude and longitude, than the usual range in pans filled with tallow. Unfortunately, however, the impression made by a ball dropping upon wood is elliptical, because of the grain, so that when the ball is replaced in the hollow it has made it does not lie securely, but is likely to roll or rock a little, making determination of its proper position somewhat difficult. The results for the night of July 10, with wood, were omitted from the final calculations.

Two possible sources of error remain to be considered, — the action of air and the faults of levelling. Great care was taken from the first to prevent or reduce draughts. The description, previously given, of the apparatus shows that chinks at the releasing end were stuffed with cotton, if not otherwise closed. The cotton may have been slightly displaced at times, but certainly there was very little freedom of circulation through the top inclosure just before or just after a ball was dropped. The cloth tube (*c* in Fig. 1) was continuous, being woven in one piece, and at its lower end the chink about it was stuffed with cotton. Below this tube the balls fell about two meters through air inclosed only by the walls of the constant temperature room, which is about 6 m. long, 4 m. wide, and 3 m. high. The one door leading into this room was closed while the balls were falling. Two incandescent electric lamps of eight or sixteen candle power were available for lighting the room; but usually only one of them was in use at a time. These were suspended in such a way as to light up the plumb-line and the brass tip over the balls to advantage,

one hanging nearly over the line of sight of telescope No. 1, the other nearly over the line of sight of telescope No. 2, the one nearest to the line of fall being, perhaps, 35 cm. distant from it. The question whether one of these lamps could by its heating effect maintain a circulation of air sufficiently brisk to affect the course of the descending balls appreciably was often in my mind. I found, however, by trial, that nowhere was the *horizontal* current caused by the lamp strong enough to show any effect on the most sensitive anemometer at my service, — an instrument made by Hicks, of London, for which the correction for friction is given as 30 feet per minute. For a short distance directly above the lamp the *upward* current was able to keep the vanes in motion; but it is evident that the disturbance of the air by the heat of the lamp is very slight along the line of fall of the balls, altogether too slight to produce in the very little time, perhaps 0.1 second, during which the ball is falling through the free air of the room, a horizontal deviation discernible in this investigation.

It seemed quite possible, however, that the circulation of air due to the lamp might affect perceptibly the position of the lower end of the plumb-line. The weight of the ivory plumb-ball being about 82 gms. in water, and the length of the suspension being 23 m., the horizontal force which, applied at the lower end of the suspending wire, would have kept it 1 mm. aside from its proper position, is less than 4 milligrams. Careful experiment showed, however, that the heat of a 16 c. p. lamp placed about 30 cm. from the wire, not far from its lower end, had so little effect on the position of the wire that I could not make sure of perceiving the deflection. It was, apparently, less than 0.001 cm.

But the very motion of the falling ball must have produced within the long cloth tube a very considerable disturbance of the air. Could such a disturbance affect the line of fall? If the tube were exactly cylindrical and the line of fall strictly along its axis, the symmetry of the conditions would answer this question in the negative. The tube was nearly cylindrical and the line of fall coincided nearly, but not perfectly, with its axis. How much deflection would be produced by a slight eccentricity of this line of fall? This question I found myself unable to answer without special experiments, which accordingly were undertaken. October 15, 1902, I dropped twelve balls with the spout displaced about 10 cm. toward the south, and twelve balls with the spout displaced about 10 cm. toward the north. The first set were deflected on the average about 0.18 cm. towards the south and the second set about 0.46 cm. toward the north, a mean deflection of 0.32 cm. away from

the nearer side of the spout.* It is possible that in these trials, at some points of the fall, the clear distance between the ball and the nearest part of the spout was not more than 5 cm. On October 18 a set of twelve balls was dropped with the spout 5 cm. north from its original symmetrical position, and another set of twelve balls with the spout 5 cm. south from its symmetrical position. The first set fell about 0.004 cm. north and the second set the same distance south from the plumb-line, a mean deflection of 0.004 cm. away from the nearer side of the spout. On October 20 the experiments of October 18 were repeated with the same displacements of the spout, but on this last trial an average deflection of about 0.04 cm. away from the nearer side of the spout was observed. We have, then, according to these not very accurate tests:†

Eccentricity in spout.	Deflection due to eccentricity.
10 cm.	0.32 cm.
5 cm.	0.02 cm.

During the general course of the experiments the eccentricity of the line of fall was probably less than 1 cm., and, during the latter part of the time at least, it was in such a direction as to produce a tendency toward northerly deviations of the balls. This tendency must have been very slight indeed, however; too slight to deserve further consideration.

The possible errors from inaccurate levelling fall naturally into two classes: 1st, those which are made at the sending apparatus; 2d, those made at the receiving apparatus. The errors of the first class are due to the fact that, whereas the position of the *plumb-line* at its top is determined by the position of the "beak" (r in Fig. 2), the position of the *ball* just before release is determined by its contact with the lower end of the central hole in the release plate. The beak is about 4.5 cm. above the centre of the ball in position, and any considerable error in levelling would evidently put the tip of the beak a perceptible distance to one side from the vertical through the centre of the ball. Much care was given

* Newton, in suggesting to Hooke a study of the course of falling spheres, warned him that "in a narrow well the bullet possibly may be apt to receive a ply from the straitened air neare the sides of the well, if in its fall it come nearer to one side than to another."

† In these experiments of October, 1902, lead was used instead of tallow as the receiving medium. The individual variations of position of the fallen balls were perhaps as great as when tallow was used; but with lead twelve balls could be received in one pan, and changes of temperature at the foot of the tower were no longer troublesome. In further experiments I should use lead.

to this levelling; but even a careful observer may have the habit of setting the bubble a little to one side of its proper position, always to the right, let us say; and against error from such a habit the device of turning the plate through 180° in the course of the observations is no safeguard. If the observer could have moved about so as to be half the time on the east and half the time on the west of the apparatus, his trick of the eye could have been eliminated from the average conditions governing the experiments; but this was not the case. Some of the levellings were made by my assistant and some by myself, and accordingly it became necessary to study the "personal equation" of each of us in the use of the levels. This I did July 29, 1902, when it appeared that both had the habit of setting the bubble too far to the right, which would carry the plumb-line to the south of the release position of the balls and therefore tend to cause an apparent northerly deviation of the balls in falling. This effect, according to the observations of July 29, was probably somewhat more than 0.001 cm. It is not likely to have been as large as 0.002 cm.

As to faults of levelling at the receiving apparatus, the changes of position of this apparatus, and of the observer with respect to it, were such as possibly to eliminate error due to such faults from the final result; but whether it was perfectly eliminated, I have some doubt, as this levelling was less careful than that at the sending apparatus. There were two places for faults, one in levelling the ring which supported the receiving pan, the other in levelling the tripod which held the tip of brass above the imbedded ball.

The latter operation was performed for every ball, and as the observer was about half of the time south, and half of the time north, of the apparatus during the levelling, his errors in this particular should very nearly eliminate each other. The levelling of the supporting ring was not so often or so systematically done. On July 24, almost at the end of my main series of observations, I found the north side of the ring to be a little higher than the south side, the inclination being, perhaps, $5'$. This would make the north side of the one inch ball fall through about 0.0036 cm. greater thickness of tallow than the south side. The effect of such a dissymmetry is rather difficult to estimate, but it seems unlikely that it would push the ball as much as 0.001 cm. toward the south.

The suggestion having been made that the copper wire used for the plumb-line might be sufficiently magnetic to suffer a slight deflection from the vertical toward the direction of the magnetic dip, I made such experiments as seemed likely to meet this suggestion. For this purpose I

placed perhaps 10 cm. of the wire, not far above the ball as the whole hung suspended in place, in a horizontal magnetic field which probably averaged, for the 10 cm. mentioned, a strength more than three thousand times that of the earth's horizontal magnetic field. I then made observations on the position of the wire with *field off*, *field on (direction north)*, *field on (direction south)*, repeating the changes a number of times. I found no effect of which I could be sure, although the conditions were such as to make the magnetic deflecting action on the wire perhaps twenty or thirty times as great as the corresponding action which could have been exerted by the earth's magnetic field during the ordinary course of the experiments.

An attempt to measure approximately the magnetic deflection of an iron wire, substituted for the copper wire in the artificial magnetic field, failed, for the reason that a bit of thread, used to piece out the suspension at the top of the iron wire, by its continual untwisting kept the plumb-ball in a state of slow rotation, which produced a deflection unless the ball happened to be placed in a perfectly symmetrical position in the vessel containing the water surrounding it. The rotating ball acted as if slightly repelled by the nearer side of the vessel.